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Gayatri Sarin
Macalester College

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Section 1.1 Introduction

The perception that economics no longer fits squarely into the domain of the social sciences enjoyed a brief bout of attention from philosophers of science in the early 1980s. Thereafter, interest in the scientific status of economics has waned, on at least one account, because economists 'are not particularly anxious about the scientific respectability of their discipline' (Rosenberg 1983). The title of the article by Gordon (1978) "Should Economists Pay Attention to Philosophers?" (his answer is a firm 'no') is telling of the attitude to the debate, and the reluctance of philosophers and economists alike to participate in it. Nevertheless, economic theories are powerful tools that shape the landscape of politics and so deserve to be scrutinized by philosophers of science.

This paper evaluates some of the criticisms leveled against the methodology of economics in order to discuss whether economics can ever attain a status comparable to that of the natural sciences. The aim is to paint a picture of what modern economics is by comparing it with historical developments in the sciences. It is my contention that the problems economics faces are the same as those historically faced by the physical sciences and if they differ, they do so in degree alone, and not in kind. Further, the fact that economics deals with human beings may forever closet it in the social science buildings, however, the empirical rigor of its claims can, in principle, be at par with those of the physical sciences.

Section 1.2. Science and Demarcation Criteria

For the purpose of this paper, I will draw on the work of Thagard to discuss some general criteria for evaluating the scientific status of a discipline. His principles will provide the structural framework of my defense of the empirical status of the subject of economics. I have selected his principles because they build on the strengths of Kuhn's, Popper's and Lakatos' demarcation criteria, and outline some generally agreed upon characteristics of good science. Though none of the following criteria guarantee a theory a place in scientific community, when taken together they provide a fairly accurate picture of how science should be: Briefly, for a discipline to be considered scientific, it needs to be:

1. Verifiable – 'A theory is said to be verifiable if it is possible to deduce observation statements from it' (Thagard 1978). The derivation of observation statements links to the second criterion - falsification.
2. Falsifiable – Through experimentation, a theory or its claims must be potentially falsifiable. Though falsification can sometimes only occur once a better theory comes along, the proliferation of modifying auxiliary hypotheses to "save" a theory reveal a theory's lack of falsifiability.
3. Have an appropriate 'Theory, Community and Historical Context' – 'theory' delineates the familiar elements of 'structure, prediction, explanation and problem solving' of the heuristic apparatus of the subject. Thagard puts special emphasis on the nature of the community of practitioners of a discipline in that he asks, 'Are the practitioners in agreement on the principles of the theory?'; 'Are they actively involved in attempts at confirming and disconfirming

> their theory?' (Thagard 1978). If one answers no the above questions, the theory in question should be considered pseudo-scientific. Even if the theory fulfills this criterion, it could fail to be scientific if it has been less progressive than alternative theories over a long period of time (historical context), or if there has been little effort to increase the scope and accuracy of its explanations (progressiveness).

With these criteria in mind, I proceed to investigate the claims made against economics, in light of its alleged failure to qualify as an empirically scientific discipline.

Section 2.1. – Verifiability and Controlled Experiments

In Philosophy of Natural Science, Carl Hempel discusses the importance of testability in the demarcation between science and non-science. He rejects the idea that a theory without testable implications can ever be considered to have scientific content:

> If a statement or set of statements is not testable at least in principle, in other words, if it has **no** test implications at all, then it cannot be a scientific hypothesis or theory, for no conceivable empirical findings can then accord or conflict with it. – Hempel Philosophy of Natural Science

If there exists no encounter of a theory with the empirical world that can potentially verify its claims, the theory must be tossed out as non-science. The first objection to the status of economics as a science deals with the heuristic apparatus of the subject.

Edward Leamer, an influential economist, acknowledges the characteristic preponderance of debate and scarcity of agreement between the various schools of economic thought. The prolonged limbo between acceptance and rejection of theories in economics is linked to this first major criticism of the subject: Its inability to produce controlled experiments. For the predictions of any science to bear any conviction, they need to be testable in the empirical world. It has been argued that the methodology of economics, making sweeping assumptions that – notwithstanding reality - are difficult to test out, guards it from ever being falsified through experimentation. Economists regularly jump to the task of making policy recommendations, 'despite the lack of a single accepted theory' (Leamer1996) making their claims rather dubious. Experimentation is thus the crucial hurdle that is needed for a discipline that relies on subjective interpretation to convey its findings.

Historically speaking though, Astronomy and Cosmology faced similar difficulties in controlled testing but still managed to secure a position in the scientific community. Many young sciences that began with limited data have been able to extract scientific theories from the natural experiments available for study. For example, Lindzen's (1990) reference[1] to narrative of the development of the diurnal nature of tides in atmospheric sciences makes economics seem like it might be on a similar trajectory:

> For a long time theory and data leapfrogged each other, with theory postulating mechanisms on which little data were available, the data

[1] In Leamer (1996)

> becoming available and contradicting the theory and new theory then emerging. Because the amount of data was large and it was error ridden, something like what economists call reduced-form modeling went on continually in order to extract patterns from the noisy data.

Economics faces the same problems that atmospheric science once faced in that there is sometimes a natural barrier against experimentation[2]. Since the subjects of study in economics are human beings, on whom experimentation (with taxes, migration, poverty) may not be permissible, the best an economist can do is to seek out natural experiments from history to test a theory. Ideal test conditions resemble sudden shocks to the economy, like a sudden influx of immigrants, a crop failure, or a trade boom, where ceteris paribus conditions roughly. A popular joke among economists is that sometimes, "Doing econometrics is like trying to learn the laws of electricity by playing the radio" (Leamer 1983), a reference to the rudimentary tools available for economic analysis.

When economists spot powerful natural experiments, the tests of their theories become more robust. When theories hold over several different data sets and are confirmed by different economists, they gain objectivity. This methodology is well established in the medical practice, where often the assessment of evidence is statistical, or probabilistic, and theories are technically at the most only verifiable through tests, though no less scientific.

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[2] Laboratory testing, widespread in experimental economics does actually involve controlled tests. Experimental economists are a minority in the profession, but will likely be more commonplace in the future.

The debate about the effect of smoking on lung cancer developed along similar lines, where correlations between the two variables were measured in different populations to expose a causal connection between the two.

The fact that economics may seem to proceed without making testable observation statements probably comes from a misguided expectation of natural science-like experiments from the economics department. A broader perspective on the development of sciences reveals that several other disciplines faced the same problem with experimentation and verification (in differing degrees) and still attained a scientific standing.

Section 2.2 Confirmation and Diversity

Another expectation of a science is that it must not develop in isolation from the web of knowledge developing around it. The more a theory is corroborated with existing sciences, the better the chances of it being considered a science itself. On the view of Buchannan and Vanberg (1990), the retrospective approach that economics takes is very similar to the approach taken by earlier versions of evolutionary theory. I would argue that economics today might look quite similar to what evolutionary theory looked like soon after Darwin's 'On the Origins of Species'. In both disciplines, the intuition of a central unyielding trend (in evolution- natural selection; in economics- the invisible hand of the market) is used to explore and systematize a range of possible theories by seeking out experiments in the natural world. Both deal with phenomena that have strong stochastic elements that often vary rapidly over time, and neither are able to specify crucial tests. Evolutionary theory has been inducted into science because it has received confirmation from a wide range of academic disciplines. Geology, Biology, Archeology and Paleontology have all

produced evidence in support of evolutionary theory, so though none have conclusively proven the phenomena, the diversity in its sources of confirmation makes the case for its likely truth. Whether the claims put forward by economists have been confirmed in a variety of disciplines will be important to evaluating its scientific standing.

Given that most of economics does not use controlled experiments to test out theories, one might think that confirmation is a vague and subjective issue in economics. Though, accepted, the greater need for interpretation in economics makes confirmation of theories less straightforward, there are conventions in place that lay out the economist's equivalent of "good science". A good economist would reserve his judgment on a theory until it has been shown to hold in a variety of contexts, times and places. For example, expansionary fiscal policy that would work to circumvent a recession in Costa Rica, might cause hyperinflation in Zimbabwe, because the institutions there are much weaker. This indicates that results in economics are less generalizable, and may seem opaque to the non-economist. Within the discipline, however, there is a formalized standard battery of tests to confirm and evaluate a theory. Conventions such as robustness tests of errors, a decent R-square[3], a coefficient magnitude that has some meaning[4] and statistical significance at the 5% level[5], all indicate that the 'community of practitioners' is actively seeking to test and confirm economic theories in a variety of contexts.

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[3] It measures the percentage of variation in data explained by the model

[4] Sometimes coefficients are statistically significant (not 0), but are so close to 0 that in economic terms they have no meaning.

[5] There is less than 5% of a chance that the result observed would have obtained if there was no significant correlation between the variables.

Section 2.3. Scope of Explanations and Accuracy of Predictions

In 1983, when Rosenberg was writing about the state of economics as a science he argued that it failed to be a progressive research program because neither was the precision of its predictions improving, nor was the scope of its explanations increasing. These now dated criticisms provide an ideal opportunity to discuss how economics today looks very different from economics twenty-five years ago.

In terms of scope, experimental and behavioral economics have made vast contributions to the subject of economics in the past few decades. Voter turnout, behavior of the family, investments in human capital, crime and punishment, game theory, institutions, business cycles, learning curves all show that economics is lending and borrowing from the other social sciences. The scope of economics has increased so rapidly as to prompt a reactionary movement dubbed 'economic imperialism' in the other social sciences. I am a firm believer that no science that purports to explain everything should really be called a science (as per Popper's demarcation criteria). But that more aspects of human behavior can now be explained in economic terms shows that economics is becoming increasingly corroborated with other academic disciplines. The lending and borrowing of tools from statistics and mathematics[6], coupled with the creation of new subfields

[6] Another fundamental development in economics, worth mentioning is in macroeconomics. In the last few decades for the first time macroeconomists started to explicitly model time. Prior to this macroeconomics hinged on an 'enormous compression of time', making its models simple, elegant but incomprehensible in terms of how their predictions bore out in the real world (Levine 2008). The

like behavioral economics all indicate that economics is contributing to and expanding the web of science.

Section 3.1. The Empirical Status of Economics

Thus far, I have addressed some of the common objections to economic methodology. In no way do I think that economics could or should attempt to become a natural science, simply because its object of study is not the natural world. What is interesting to me is whether its predictions and theories can attain the empirical rigor accorded to the hard sciences. I have found that many areas of study that now belong to the hard sciences have initially faced difficulties like the absence of controlled experiments, the need to interpret the results with limitations in objectivity, and so these should not rule out the possibility of economics becoming a more empirical science.

A common angle taken on by denouncers of the empirical status of economics has to do with its mathematical form. A good amount of microeconomic theory is entirely deducible from a handful of axioms and a basic understanding of calculus. For example, the law of demand is deducible from a mathematical feature of the shape of utility curves (that they are concave). The core of microeconomic theory is sometimes viewed as so immutable

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dynamic modeling of time made possible a distinction between short run effects and long run effects of policy changes, making fundamental contributions to the predictive scope of macroeconomic models itself. Macroeconomic models today bear little resemblance to their elegant predecessors, a change deliberately made to increase the empirical content of the subject at the cost of conventional attributes of theories.

that its claims are in simple logical truisms that are devoid of empirical content, and so cannot be considered scientific.

Rosenberg (1984) initially articulated this objection by making the analogy between geometry and economics. He asserted that economic theory relied on a set of mathematically convenient assumptions, which had the two-fold effect of making the theory rich in conceptual clarity, but poor in empirical content. In the process of abstracting away from the complexities of human interactions, economists had landed up creating a discipline quite like Euclidean geometry for which no empirical correlates existed. He recommended that economics should thus be treated as 'a pure axiomatic system, whose terms may or may not be instantiated in the real world, but which is of great interest, like Euclidean geometry, whether or not its objects really exist' (p390).

What I would point out here, is that the chief goal of economics is to be a rich theoretical tradition. It is true that certain nuggets of economic theory seem pre-ordained to be in the economics textbooks, as if it were an unwritten rule agreed on by the profession, but undergraduate economics textbooks do not define the status of the discipline. Levine (2009) went as far as to say that 'undegraduate economics textbooks were an embarrassment to the profession' because they were not consistent with the graduate textbooks that more accurately reflect the profession as it is today. Most of the empirical developments in economics happen at the Ph.D level and do not filter down to the general public. What is perceptible to the general public is introductory economic theory and economic policy, both of which have serious disconnects from the core of the profession. An analysis of the work done by nobel-laureate types of economists would, I think, speak for the empirical

achievements of the subject, because economic theories today are merited on their explanatory power not just their mathematical elegance.

Also, the fact that economic theories are verifiable only within a certain range of error is, I think, well explained by the fact that economists study the reactions of human beings. And human beings are intrinsically erratic, and predictable with only so much certainty. Climatologists, given the nature of the phenomena they study, also have only so much certainty in their predictions. I think modern economics has more consistent predictions than climatologists do, and this warrants approval from the scientific community. A massive economic recession here and there is like every odd natural disaster that the climatologists fail to predict. Both can be studied in retrospect.

Section 3.2 Commensurability and Empirical Progress

Once a scientific theory is established, there is the expectation that predictions will have strong empirical correspondence to the real world. The physical sciences, rich in empirical content, claim to be 'carving the world at its joints', because the earlier models or theories are commensurable with their successors (at least in normal science). So for economics, it will not suffice to tell us just the logical outcomes of its models if it is to be any more than a tautological science. Rosenberg (1983) initially defends the economist by pointing to the development of heredity from the Mendelian gene.

> There are several scientific theories which to varying degrees fail to divide nature at its joints. For instance, the Mendelian unit of

> inheritance cannot be reduced to the molecular gene and so does not divide its phenomena at the joints. Yet Mendel's laws are useful approximations that we would be silly to forgo.

The purpose of models in economics 'whether mathematical or verbal' is to serve as metaphors (Leamer 1996). For Keynes, to convert a model into a quantitative formula was to 'destroy its usefulness as an instrument of thought' (Keynes 1938). Leamer (1996) sums up the matter in this oft quoted statement: 'Models are neither true nor false. They are sometimes useful and sometimes misleading'. A useful way of seeing their relation to the empirical world is to compare them to the Ideal Gas Laws. The predictions are unambiguous only on a range of phenomena, and like the Ideal Gas Laws, they break down at (the economic equivalents of) extremes of temperature, pressure or volume. Whilst many people think that the assumption of rationality is rigid in economic models, it is really only a simplifying assumption (when irrationality in either direction cancels itself out) that is suspended when required by the phenomena being studied (for example stock bubbles).

> As per Keynes' standard description of economics: "Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world. It is compelled to be this, because unlike the typical natural science, the material to which it is applied is,

in too many respects, not homogenous through time.

The final objection I would like to address comes from the end of Rosenberg's article "If economics isn't a science, what is it?". He claims that no improvement in economic theory could provide 'laws governing intentional economic activities', because the claims derived from the fundamental assumptions about the determinants of individual actions (that people act rationally) 'can be shown to follow from assumptions which are in direct denial' of the ones economists cite. His argument is that the general statements made in economics can often be shown to be derivable from different starting assumptions, some of which may be contradictory. This was certainly true of consumer choice theory prior to Bentham, which focused on deriving the same law of demand from a cardinal notion of utility[7]. Bentham decisively showed that the same microeconomic laws could be derived from an ordinal notion of utility (that consumers need only be able to order their preferences making interpersonal comparisons of utility impossible).

However, one glance at the work of Kuhn would convince anyone that this is a problem shared by each one of the hard sciences, and if present in economics, is only so to a greater degree. The very basis for Kuhnian paradigm shifts was that revolutions in the scientific world were prompted by

[7] This meant that consumers had to be able to assign a numeric value to the utility derived from different bundles of goods

sudden changes in the underlying assumptions of theory, the workhorse example being the switch from Newtonian mechanics to general relativity.

In conclusion, I reiterate that each of the objections presented to the scientific status of economics apply (or have applied) in varying degrees to other disciplines that are currently deemed scientific. None of them satisfactorily rule out the possibility that economics can in the future be considered an empirical science. This observation opens up two interesting lines of discussion. The first is that scientific status is transient and temporal in nature. Disciplines can shift between being considered scientific and pseuodoscientific over time based on the trajectory that they take up. Thagard points out that astrology – though now considered a pseudo-science, should rightly have been considered a science earlier in time because it did not fail the demarcation criteria until more recent times. One of the reasons he cites is that there weren't any contending theories that better explained the phenomena that concerned astrology until recent developments in science. This points to the second interesting observation: the line between science and pseudoscience has as much to do with developments outside of the discipline (alternative explanations of the same phenomena), as with the methodology employed within the discipline.

The methodology employed by economics, in my assessment satisfies to some degree, each of the criteria for good science. Whether economics will eventually be considered a science depends both on developments within the discipline as well as how

successful contending explanations of the same phenomena are. While I reserve my judgment on its eventual status, I assert that the arguments presented thus far do not rule out the possibility that economics can one day be at par with the other sciences.

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